to ideas differing from those involved in the "new theory," which, so far as extracts such as the above go, appears to include nothing whatever of a theoretical nature.

Even in Prof. Ostwald's letter there are, however, indications that in his book he went beyond the mere statement of the laws of solutions. For example, he says: "In my book the question is this one of facts, and although I have therein made more use of *molecular considerations* than I should at present hold to be proper, yet I have done so only to render more clear the actual relations, and never to prove quantitative laws."

Now it was solely the "development of the consequences of facts"—this use, which Prof. Ostwald admits to have been excessive, of "molecular considerations"—which has generally been, and was by me, styled the "physical theory." The facts themselves no one can question; indeed, I took pains to point out in my review that the facts, as given in the book, would alone serve to make it valuable. The theoretical matter, however, called for separate consideration. It alone was, and, so far as I can see, it alone could be, designated the "physical theory."

In denying the contrast between the so-called chemical and physical theories, Prof. Ostwald declares that he never maintained "that no interaction takes place between the solvent and the dissolved substance." If such was his opinion when writing his book, it may be asked, Why in all fairness should he have defined solutions as homogeneous *mixtures?* Why did he not state clearly that interactions between solvent and dissolved substance were possible? It is quite true that, in my review, instances were given of chemical expressions used in the book, but no stress was put upon these by the author as indicating the general occurrence of chemical changes in solutions. They seemed to arise, not because of, but rather in spite of, the author's idea of the nature of solution, and could only be regarded as inconsistencies. The theme of the book was the explanation of the properties even of concentrated solutions by considering the interactions of molecules of the *same* kind, by treating the dissolved substance as if it were gasified. If such a method of treatment were described as "physical," I think the commonly accepted meaning of the word was in no way impaired.

Indeed, much of Prof. Ostwald's book can hardly be justified if interactions of a chemical nature are probable in solutions. For instance, several pages are devoted to the use of van der Waals's equation in dealing with solutions. To anyone familiar with the deduction of this equation, the validity of its application to a solution even when the solvent is regarded as indifferent is highly questionable. If, however, it is admitted that something of the nature of a chemical reaction may occur between solvent and dissolved substance, that the latter may not be in a pseudo-gaseous condition, then the application of the equation can hardly be termed otherwise than meaningless.

In conclusion, I can only express regret if my review has tended to create further misconception on this vexed question of solution. At the same time, I hope I have been able to indicate to Prof. Ostwald the points which led to my use of the terms to which he objects; and I venture to think that in the discrepancies which appear to exist between the ideas as given in his letter, and those which the reader has to gather from his book, is to be found sufficient reason for the use of the statements to which exception has been taken. J. W. RODGER.

London, February 1.

Arrow Poison.

IN 1889 a French naval surgeon, M. Ledantec, published in the *Annales de l'Institut Pasteur* the result of some investigations he had made into the nature of the arrow poison of the natives of the New Hebrides. Wounds from these arrows give rise, as is well known, to tetanus, and M. Ledantec was able, by the subcutaneous injection of the scraped off poison, to kill guinea-pigs under typical tetanic symptoms. He learnt from a Kanaka that they are prepared by smearing the arrow-heads (which are made of human bone) first with tree gum and then with mud from a swamp, which mud he found to contain numbers of Nicolaier's tetanus bacillus.

As far as I am aware, this has been recorded only of the natives of the New Hebrides and some of the neighbouring groups (the arrow poison of Stanley's dwarfs is certainly *not* the same), and I was therefore much interested some days ago by coming accidentally upon an old record which seems to show that the natives of the Cape Verd coast were accustomed, more than three hundred years ago, to get rid of their enemies in a similar manner. In Hakluyt's "Voyager's Tales," published in 1589 (I refer to the little reprint edited in 1889 by Henry Morley), is the narrative of one Miles Phillips, in which occurs the following passage:—"Upon the 18th day of the same month (November 1567) we came to an anchor upon the coast of Africa at Cape Verde, in twelve fathoms of water, and here our General landed certain of our men, to the number of 160 or thereabouts, seeking to take some negroes. And they, going up into the country for the space of six miles, were encountered with a great number of negroes, who with their envenomed arrows did hurt a great number of our men, so that they were enforced to retire to the ships, in which contest they recovered but a few negroes; and of these our men which were hurt with their envenomed arrows, there died to the number of seven or eight in a very strange manner, with their mouths shut, so that we were forced to put sticks and other things into their mouths to keep them open." In the language of modern medicine, they succumbed to tetanus traumaticus. The voyagers left the coast soon after, and there is no further mention of the natives or of the wounded.

There is, of course, no proof that the arrows were poisoned with mud or earth, but the probability is considerable. The chief interest lies in the age of the record, which forms in some manner a pendent to the researches of M. Bossano (*Comptes rendus*, 1888), which showed the tetanus bacillus to have a very wide distribution in space.

It is a curious consideration that this and the other famous arrow poison, curare, both kill by their action on the voluntary muscles, the action of one being diametrically opposed to that of the other. A. COPPEN JONES.

Davos Platz, Switzerland, January 30.

The Implications of Science.

HITHERTO prevented from again writing, I cannot now remain passive and allow Mr. Dixon to escape from his irrational position under cover of a cloud of verbiage—like a cuttle-fish through water made turbid by its ink.

In my lecture I pointed out that certain truths are implied in all physical science. They *are* so implied. If Mr. Dixon thinks they are not, it is for him to show how experimental science could be carried on, with any real, serious doubt about them. This he has certainly not yet done.

Our knowledge of our own existence "in the present," is knowledge of a particular concrete fact, not of an abstract necessary truth. That "whatever feels, simultaneously exists," *is* such a "necessary truth," but it is an abuse of language to apply that term to anything which may cease to exist the moment after its existence is recognized.

That "nothing can simultaneously be existent and non-existent," does not at all depend upon "terms" or "definitions," but is a law of "things." It would not lose its validity and objective truth, not only if there were no such things as "terms" and "definitions"; it would not lose it if the whole human race came to an end.

I am glad to find my critic does "not doubt" that if he lost an eye his condition would thereby be modified, but if he does not also see that this applies and must apply everywhere and everywhen, I do not envy him his power of mental vision.

Oriental Club, February 2. ST. GEORGE MIVART.

The New Forest in Danger.

IN connection with my letter which appeared in NATURE of the 28th ult. (p. 295), it may interest some of your readers to know that the petitions, to which I referred, in support of the Bill for excepting the New Forest from the operation of the Ranges Act, 1891, have already been signed by Lord Walsingham, F.R.S. Prof. C. Stewart (President of the Linnean Society), Sir Joseph D. Hooker, F.R.S., Dr. P. L. Sclater, F.R.S., Mr. Osbert Salvin, F.R.S., Dr. A. Günther, F.R.S., Dr. H. Woodward, F.R.S., Mr. W. Carruthers, F.R.S., Dr. D. Sharp, F.R.S., Mr. Thiselton-Dyer, C.M.G., F.R.S., Mr. H. W. Bates, F.R.S., Mr. F. DuCane-Godman, F.R.S., Dr. G. Buchanan, F.R.S., Dr. B. Richardson, F.R.S., Prof. J. O. Westwood (Professor of Zoology, Oxford), Dr. Thorne-Thorne, F.R.S., Mr. J. G. Baker, F.R.S., Mr. W. H. Preece, F.R.S., Mr. Botting Hemsley, F.R.S., Mr. E. B. Poulton, F.R.S., Mr. R. McLachlan, F.R.S., Mr. C. B. Clarke, F.R.S., Major-General Carden, Prof. Jeffrey Bell (Secretary of the Microscopical

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Society), Dr. Franklin Parsons, Mr. Daydon Jackson (Secretary of the Linnean Society), Mr. J. E. Harting, Dr. Bowdler Sharpe, Mr. J. Britten, Mr. E. Saunders, Colonel Swinhoe, Mr. A. W. Bennett (Vice-President of the Linnean Society), Mr. Percy Sladen (Secretary of the Linnean Society), Mr. D. Morris, Mr. Miller-Christy, and by a large number of other Fellows of the Linnean, Geological, Zoological, and Entomological Societies of London; and by the editors of the *Geological Magazine*, the *Journal of Botany*, the *Zoologist*, the *Entomologists' Monthly Magazine*, and the *Entomologist*. HERBERT GOSS.

Linnean Society, Burlington House, W., February 6.

ON THE NEW STAR IN AURIGA.

WE were enabled last week to make an announcement of the discovery of a new star in the constellation Auriga, as we received on Wednesday the Edinburgh Circular giving an account of the manner in which the first information had been received. A telegram was sent by Dr. Copeland to the Astronomer-Royal on the date of the reception of the post-card—Monday, February 1—and, as we have since learnt from the *Astronomische Nachrichten*, a telegram was also sent by Dr. Copeland and the Astronomer-Royal to Kiel. Unfortunately there is at present in England no local system for the distribution of astronomical intelligence of this character, so that it will probably be found that the fine night of Monday was only devoted to observations of the new star in a very restricted number of Observatories. The necessity for correcting this state of things has been pointed out by Mr. Lockyer in the *Times* of Friday, and it is to be hoped that some steps may be taken to rectify the defect. As it turns out, however, no very great harm has been done, for the new star, instead of degrading its light rapidly from the day of its discovery on February 1, seems if anything to have brightened, so that the changes in its light between Monday and Wednesday were probably not so great as those observed in Nova Cygni during the first two or three days of its visibility.

A telegram from Prof. Pickering communicated by the Astronomer-Royal to the *Times* of Monday seems to show that the star, instead of bursting forth suddenly about the date when the anonymous post-card was sent to Dr. Copeland, has really been visible since last December, perhaps even for a longer time; but in any case it has not been registered in any recognized Catalogue. Prof. Pickering states that he finds this star visible on three plates belonging to the series of the Draper photographs at different dates during the month of December. The telegram through the "Centralstelle für Astronomische Telegramme," Kiel, runs as follows:—"Copeland's Nova bright on photograph December 10, faint December 1; *maximum* December 20; spectrum unique.—PICKERING." It would thus appear that Prof. Pickering had photographed the new star on the three dates named in the course of the photographic mapping of stars and their spectra which he is carrying out at Harvard College Observatory. We do not yet know whether the plates were examined at the date on which they were taken, or whether the telegrams relating to the appearance of the Nova may have caused an examination to be made, but the spectrum was visible on all three plates.

As we stated last week, the observations in England commenced on Monday night at Edinburgh, on which date Dr. Copeland saw bright lines, and at the Royal Observatory on the same evening, when the new photographic 13-inch refractor was used for determining the exact position of the star. With this fine instrument the Astronomer-Royal was able to detect that the star differed from the other stars on the plate in appearance.

As to the work on Tuesday night, at present we know nothing. An announcement of the discovery appeared in the *Times* of Wednesday, on which date also, as we have already stated, Dr. Copeland's Edinburgh Circular was received in London. On that night, therefore, which happened to be fine, observations were commenced at South Kensington, and two photographs were obtained, together with some eye-observations, which were communicated to the Royal Society by Mr. Norman Lockyer on the next day in a preliminary note, from which we make the following extracts:—

"Last night was fortunately fine, and two photographs were taken of the spectrum—the first exposed from 7.30 to 9, or for 1h. 30m.; the second exposed from 9.30 to 12.30, or for 3h. 0m. The first registered 13 lines; the second appears to contain some additional ones, but they are very faint, and have not yet been measured.

"A complete discussion of these photographs will form the substance of a subsequent communication, but already the following approximations to the wave-lengths have been obtained, the photographs being treated absolutely independently, means, however, being taken for the four least refrangible lines, as there has not yet been time to construct a proper curve for this region:—

Lines measured in the first photograph.			Lines measured in the second photograph.		
Wave-length.	Hydrogen lines.	Probable origin.	Wave-length.	Hydrogen lines.	Probable origin.
3933 K		Ca	3933 K		Ca
3968	H		3968	H	
4101	*h*		4101	*h*	
4128			4130		
4172			4172		
4226		Ca	4227		Ca
4268			4268		
4312		Hydro-carbon	4310		Hydro-carbon
4340	G		4340	G	
4516			4516		
4552			4552		
4587			4587		
4618			4618		

"I have recently taken up the question of stellar spectra, and find that a 6-inch object-glass with a prism in front of it is all that is required for the brighter stars. This instrument was employed upon the Nova, which is of about the fifth magnitude, so the exposures were necessarily long.

"For the eye-observations, the new 3-foot mirror which has recently been presented to the Astro-Physical Laboratory by Mr. Common was employed, but unfortunately the clock is not yet mounted, so that the observations were very difficult.

"C was the brightest line observed. In the green there were several lines, the brightest of which was in all probability F, the position being estimated by comparison with the flame of a wax taper. Another line was coincident—with the dispersion employed—with the radiation at wave-length 500 from burning magnesium wire. A fainter line between the two last-named was probably near λ 495, thus completing the trio of lines which is characteristic of the spectra of nebulæ. There was also a fairly bright line or band coincident with the edge of the carbon fluting at λ 517 given by the flame of the taper. A feeble line in the yellow was coincident under the conditions employed with the sodium line at D.

"The hydrogen line at G was distinctly seen, as well as a band, or group of lines, between G and F.

"Nearly all the lines appear to be approximately, if not actually, coincident with lines seen in the various types of Cygnus stars, the chief difference being the apparent existence of carbon, hydrocarbon, and calcium in the Nova.

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